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R E M A R K S

ON

MR JOHN BELL'S

A N A T O M Y

OF THE

HEART AND ARTERIES.

By JONATHAN DAWPLUCKER, Esq.

(Ino. Barclay m.d.)

By ignorance is pride increas'd :
Those most assume who know the least ;
Their own false balance gives them weight,
But every other finds them light. GAY.

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HEART AND ARTERIES

BY JOHN W. BARTON, M.D.

It is generally to be expected that
those who are most anxious to know the
truth of their own state, should be
able to see every other state of the

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DEDICATION
to
JOHN BELL, Esq.

SURGEON IN EDINBURGH.

SIR,

THE second volume of your very valuable work on Anatomy happening, soon after its publication, to fall into my hands, I wrote, by way of amusement, a few remarks upon it, without any intention of giving them to the public. But when I was some time ago told, that you had declared publicly in your class that you valued and esteemed truth above every other consideration; that, in consequence of this declaration, you had attacked the most respectable characters; and that you had not even spared your brethren of the same profession—I no longer doubted that you would consider the publication of my remarks, how trifling soever they may be, as a very particular favour. And as I have always been one of your most ardent and most devoted admirers ever since I had the felicity of being acquainted with your great name, I immediately resolved to gratify you in this particular; considering that I would at the same time be promoting the cause of truth, for whose interests I am no less

A

zealous

zealous (pardon the comparison) than your illustrious self. I have, Sir, accordingly published them out of pure good nature and zeal to oblige you; and I thought I could not possibly do better than dedicate them to the celebrated man whom they most concern, and to whom, I am persuaded, they will give the greatest satisfaction and delight.

I HAVE no doubt, Sir, that you, who have taken upon yourself the vindication of truth, and who have exerted yourself already so ably and so disinterestedly in her cause, will embrace the earliest opportunity of reading these my remarks publicly in your class, and of recommending them to the careful perusal of all your pupils. You may perhaps cause them to be bound up with your second volume, that none of your readers may find the least difficulty in coming at the truth. But I would take the liberty to hint, that this would not be altogether proper; for it would be bestowing an honour upon my poor performance to which it is by no means entitled.

I INTENDED at first to have published my remarks on the second part of your volume; but when I recollected that almost the whole of that part, even your discoveries, have been taken from Haller and Sabatier; and that the observations of your own, which now and
then

then occur, are of very little importance, I changed my mind. But if I find you sufficiently grateful for this present favour of mine, I may perhaps, at some future period, gratify you with my remarks, not only on the remainder of your last volume, but on all your other publications.

As I am one of your most devoted and zealous admirers, nothing can give me greater pleasure than to hear of your success in life. Allow me therefore, before I conclude, to give you a hint or two, which you may perhaps find useful.

ONE thing you should particularly aim at, I mean, to be attacked publicly by some eminent man; because you might then, with great propriety, cry out *persecution*. You would raise a party in your favour, and your success would be infallible. You should therefore attack the characters of the most respectable men of the same profession with yourself; you should treat them on every occasion as a parcel of fools and knaves, and declare that their writings contain nothing but lies and absurdities. The farther your assertions are from the truth, the more apt will these respectable characters be to attack you; and in that case you would gain your point. You would have only to represent their attack as proceed-

ing from envy at your superior abilities and skill, and from a desire of concealing from the world your merit, which if sufficiently known would destroy themselves. This plan you have indeed followed; but I do not think you have gone far enough. These gentlemen are too prudent and too good-natured to retort, or perhaps they are too proud to spend even a thought upon you.

WHAT do you think, therefore, of affirming, that my remarks have been written by some of these medical gentlemen out of pure spite and ill-nature, in order to tarnish those laurels which they could not hinder you from obtaining, and to diminish that glory which they could not rival? Upon very mature deliberation, I consider this as the best plan which you can follow.

You can easily make a very *pathetic* speech on the subject: You may say also, that this *unfortunate book* of mine fell into your hands by accident; that it is a *filthy pamphlet*; that the remarks which it contains are exceedingly *filly* and *trifling*; that the person who wrote them *knew nothing* of the matter; that he had displayed the daring and unpardonable ambition of being transmitted to posterity as the antagonist of the illustrious philosopher, whose name shall shine with

an eternal lustre, and who shall be known, and admired, and adored, in those ages when Newton shall be forgotten, and the sun, and the moon, and the stars, to use your own sublime language, are gone to the vault of all the Capulets.—I am,

Illustrious SIR,

Your sincere Friend,

And devoted Admirer, till death,

JONATHAN DAWPLUCKER.

CONTENTS.

in eternal light, and who shall be known, and ad-
mired, and beloved, in those ages when Newton shall
be forgotten, and the sun, and the moon, and the
stars, to the very language, are gone to



the year of our Lord 1691

William St.

Your Obedient Son,

And devoted Admirer, till Death.

JOHN THOMAS DAWKINS.

CONTENTS.

CONTENTS.

CHAP. I.

Of the heart, p. 9.—Valve of Eustachius, p. 11.—
Coronary vessels, *ibid.*—Anatomical remark, p. 12.—
Style, *ibid.*—Grammar, p. 13.—Spelling, p. 16.

CHAP. II.

Leeuwenhoeck's theory of the red globules, p. 17.
Excellent argument of the author, p. 20.—His great
chemical discoveries, p. 23.—Wonderful qualities of
ferum, p. 25.—Ignorance of the older chemists, p. 26.
Important discovery of the author, p. 28.—Older and
younger chemists, who, p. 29.—Importance of John-
bellation, p. 30.—Negative and positive, p. 32.—
Double, p. 33.—Universal, p. 34.—Individual, p. 35.
Author's great learning, p. 37.—Ancients not 30 years
old, p. 38.—Respiration, p. 39.—Heat of animals,
p. 42.—Crawford ingeniously refuted, *ibid.*—Respira-
tion of plants, p. 47.—A looking glass, *ibid.*

CHAP. III.

The interesting and short chapter, p. 49.—Lungs,
ibid.—Respiration of birds, p. 50.—Ancient and mo-
dern mistakes corrected, p. 51, 52.—Respiration of
animals which do not respire, which are amphibia and
not amphibia, p. 51.—Instance of the true sublime,
ibid.

ibid.—Discoveries concerning frogs, p. 52.—Swammerdam's section, p. 53.—Difficulties never before declared nor investigated, *ibid.*—A proclamation, p. 54. A tale, p. 55.

CHAP. IV.

Why the blood is not sent through the lungs of the foetus, p. 56. Ductus venosus admirably described, *ibid.*—Circulation in the foetus, p. 57.—Great mistakes of physiologists, p. 58.—Collapse and over-distension of the lungs, p. 59.—Author's great knowledge of Dr Hooke, p. 60.—Remarks on part second, p. 62.—Beautiful and sublime descriptions and Johnbellations, p. 63.

It is requested of the Reader to alter with his pen the following

ERRATA.

In page 10. line 11. from the top, (P. 53.) should be (P. 43.)

In page 15. last line, (P. 109.) should be (P. 9.)



REMARKS

REMARKS

ON

Mr JOHN BELL'S ANATOMY

OF THE

HEART AND ARTERIES.

ON CHAP. I.

THE following remarks were made during the perusal of Mr John Bell's Anatomy of the Heart and Arteries. As they are rather unconnected, the writer has not been very solicitous about their arrangement.

To begin with the first chapter, which is entitled,
OF THE MECHANISM OF THE HEART.

The description of the heart is in general accurate and lively; at the same time, it must be acknowledged, that the author has been more solicitous to amuse than to instruct his readers. This solicitude has probably occasioned the extreme diffuseness of the style so conspicuous in every part of this volume, and has induced him to introduce so great a number of foreign topics, that the digressions occupy no inconsiderable part of the book. Thus in the present chapter we have the disputes about the water which issued from our Saviour's side, the history of the bone of the heart, and a long account about big hearts and little hearts.

B

Perhaps

Perhaps also the author has been too eager to raise himself in the opinion of his readers at the expence of others; at least, it is not easy to see any other reason for the harsh language which he constantly uses when he speaks of preceding writers. In his account of the irritability of the heart, he says, "Philosophers have been
 "so bewitched with the desire of explaining the phe-
 "nomena of the human body, but without diligence
 "enough to study its structure, that from Aristotle to
 "Buffon it is all the same, *great ignorance* and *great*
 "*presumption.*" (P. 53.)—Such an affirmation would have been improper in any writer, because it is contrary to truth; but it is doubly improper in the present writer, because every thing which he says about the irritability of the heart is contained in the writings of those very philosophers whom he thus vilifies. Every thing which he says on that subject may be summed up in these two propositions:

1. The heart is stimulated to contract by the blood.
2. The heart contracts by a *vis insita*.

The first of these opinions was maintained by Lancisi, Senac, Whytt, &c. Haller not only maintained it, but proved its truth by a series of experiments; and it has been long almost universally received by physiologists.

The second proposition is merely the opinion of Glisson, &c. as new modelled by Haller. As our author has not attempted to refute the direct arguments brought against it by Whytt, Monro, and other celebrated philosophers, and has not brought a single new proof in order to support it, he will not be surprised



prised to hear that many of his readers are disposed to call it in question.

When treating of the valve of Eustachius, our author says that no good plate had ever been given of it except his own; yet some pages after he mentions a beautiful plate of it by Cowper, and he has forgotten altogether the plate of Haller. He tells us, that the use of that valve is still imperfectly understood; and then he proceeds to inform us, that it serves merely to complete the auricle. Now *this very use* has been assigned by Haller in a book which our author quotes frequently, and which consequently one would naturally suppose that he *must* have read.

When treating of the coronary vessels, he says: "Thebesius believed that there were some shorter
"veins, by which the blood was returned, not by a
"long circle into the right auricle, but directly into
"the ventricles of the heart. Veussens, Thebesius,
"and others who belonged to their party, pretended
"to prove this fact by injections: But what doctrine
"is there which such clumsy anatomy and awkward
"injections may not be made to prove? They used
"mercury, tepid water, and air; and they forced
"these, the most penetrating of all injections, till they
"exuded upon the inner surface of the heart; but
"using any coarse injection, as tallow or wax, the
"injection does not exude this way, but, following
"its natural course, keeps within the arteries and
"veins, and sometimes finds its way back to the au-
"ricle of the heart." (P. 28.)—Is it not natural for the reader to suppose, that our author, when he speaks in this style, has actually read the dissertation of Thebe-

sius? Yet Thebesius informs us in page 15. of his dissertation, that he made use of these *very course* injections of which our author speaks.

Though the anatomical part of this chapter be in general accurate, there occur a few passages in it which it may be proper to mention. In page 49, the author tells us, that he supposes the pericardium surrounds the heart closely; because when the heart is injected before the pericardium be opened, that covering is completely filled. Now anatomists know, that both the heart and arteries may be swelled out by injection much beyond their natural size; and that therefore the bulk of the heart, after being injected, is no proof that it filled the pericardium in the living body. In page 38 he informs us, that all the fibres of the heart are oblique; yet in the same page he affirms that some of them run nearly transversely; and in the next page, that any attempt to extricate the fibres of the interior part of the heart, and consequently to ascertain their position, is absurd and impossible. If so, how comes he to know that all its fibres are oblique? In page 54 he denies that water is ever found in the pericardium of those persons who have died suddenly, provided they be dissected immediately after death. Haller, who certainly dissected many more such persons, affirms the very reverse.

As to the style, it is entitled to the praise of being lively and entertaining; but it is rather like the style of a female romancer than a man of science. To elegance, or even neatness, it has no pretensions: the sentences are almost all ill constructed, and vulgar phrases and improper expressions occur very often.

But

But most probably elegance was not our author's aim; and if he was only anxious to appear perspicuous, he has in some measure succeeded: but by no means completely; for the language is too vague, and the sentences too confused, to bear examination. Many passages have scarcely any meaning at all. Some passages flatly contradict others; and even blunders in grammar, as the following passages will show, are not uncommon.

"In both ventricles this is very remarkable, that towards the opening of the auricle *it* is very rugged." (P. 18.)—"The little horns or tags *becomes* so tense." (P. 19.)—"They *prevent* the valve *being* forced." (P. 24.)—"To *prevent* it *gravitating* upon that which is rising from the liver." (P. 31.)—"How *terrible dangerous* it was to open an artery." (P. 61.)—"The Harveian doctrine had no sooner breathed life into the new philosophy of the human body, *or* physicians *begun* to think of the heart." (P. 67.)—"Nor can I believe that there is any difference *among* all the three." (P. 98.)—"If oil, mucilage, water, or any other fluid, be *substituted* to the serum." (P. 99.)—"Many things are to be *taken in* the calculation." (P. 125.)—"The function of the placenta actually is *equivalent* with the function of the lungs." (P. 186.)—"Something *equivalent* with the function of the lungs." (P. 188.)

The following passages, and many more might have been selected, have either no meaning at all, or a very absurd one.

"The discovery of the circulation of the blood has been always regarded as one of the grandest in science:

"science: it has been ranked rather with the great
 "doctrines of philosophy, than with the little disco-
 "veries in our peculiar science; and it has been
 "boasted of by our countrymen, and much coveted,
 "and often claimed, by strangers. Indeed its real
 "importance *falls little short of the feelings* which all
 "these disputes convey to the mind; *for* it is in itself
 "most **INGENIOUS AND BEAUTIFUL**; *and* it is the
 "foundation of all that physicians have thought or
 "practised, right or wrong, useful or destructive, ever
 "since that day." (Pref. p. 1.)

"We have trodden down *at once all their doctrines*
 "and principles. The chemistry of the present day
 "is no more like theirs than our reasonings are. If
 "we speak *now of mechanics, we mean* simply the
 "mechanism of the *human body*." (Pref. p. 8.)

"It is *peculiar* in this chiefly, that the *forms* of
 "the arteries and veins of the heart itself are *beauti-*
 "*ful*, and that the arteries rise just under the valves
 "of the aorta." (P. 25.)

"Their form they preserve only while in the blood,
 "and seem to be *supported* more by the *qualities* of
 "the *serum* than by their *own properties*; *for* if
 "mixed with water, they mix easily, and totally dis-
 "solve; the water is red, but the globules are gone."
 (P. 90.)

"And this above all is a most singular property of
 "the serum, that it *admits freely* the air to pass
 "through and impregnate the blood; for when the
 "coagulum of the blood is drowned deep in its se-
 "rum, if *turned up and exposed to air it reddens*;
 "which, if oil, mucilage, water, or any other fluid,
 "be

"be substituted to the serum, it will not do."
(P. 99.)

"Modern chemistry proves to us, that it is not the
"loss of any principle that *endows* a metal, for ex-
"ample, with *negative* powers; but the direct acqui-
"sition of a *new* principle, which endows it with *po-*
"*sitive* powers." (P. 106.)

"Water has all the appearance of a pure and simple
"element, but it is in truth a compound body, con-
"sisting of two parts; of *inflammable* AIR for its ba-
"sis, and of oxygene combined with it, in that *great*
"*proportion which the great appetite of inflammable*
"*air requires*: and as inflammable air, when satura-
"ted with oxygene, forms not any acid air, but pure
"water, it has changed its name, and is now called
"hydrogene air." (P. 129.)

"This is the reason that when many small fishes
"are inclosed in a narrow glass, they all struggle for
"the uppermost place, *as in the Black-Hole*; and
"that when in winter a fish-pond is entirely frozen
"over, you must break holes for the fishes, not that
"they may *come and feed*, but that *they may come*
"*and breathe*; without this, if the pond be small,
"they must die." (P. 153.)

"Its structure is strong, muscular, and *continually*
"*active*, performing the office of a second heart.
"The aorta, when dilated, in nine of ten cases is co-
"vered with white spots; it is diseased; *they are*
"*aged people*, and almost always the dilatation be-
"gins from the heart." (P. p. 246, 247.)

Of single words, let the following instances suffice:
Extremest vessels (p. 109.) A *mechanical* and fixed
disease

disease (p. 223.) But the word which our author has treated with least mercy is *transparent*. We have the heart of a fish as *transparent as a bubble of water* (p. 12.)—*transparent* veins (p. 14.)—the bones becoming *transparent* in old age (p. 32.)—the lungs of a crocodile very *delicate and transparent* (p. 150.)—the outside membranes of the lungs of a frog as *transparent as a soap bubble* (p. 150.)—the lungs of the ask *exquisitely transparent* like the swimming bladder of a fish (p. 150.)—and the valve of the foramen ovale *perfectly transparent* (p. 181.)

What meaning the author has affixed to the word *transparent*, or whether he has affixed to it any meaning at all, we cannot pretend to decide.

While upon the subject of words, it may be proper to mention, that there are several names which our author constantly spells wrong. We have *Eristratus* in page 60 instead of *Erasistratus*; *Vieussens* is always spelt *Veussens*; *Valsalva* is spelt *Vasalva*; and *Drellincurtius* is spelt *Drellincartius*.

Bad grammar and bad spelling, it must be confessed, are rather singular phenomena in the writings of an author, who tells us that he is acquainted with *all the philosophers from Aristotle to Buffon*.

REMARKS

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ON

MR. JOHN BELL'S ANATOMY

OF THE

HEART AND ARTERIES.

ON CHAP. II.

THE second chapter treats OF THE APPEARANCE AND PROPERTIES OF THE BLOOD, OF THE CHEMISTRY OF OUR FLUIDS, AND OF THE INFLUENCE WHICH AIR HAS ON THEM. This chapter is very long, and will require a more particular examination than the last.

The blood is commonly considered as consisting of three parts; the red globules, the gluten, and the serum. This is the division which our author adopts.

Our author's account of Leeuwenhoeck's theory of the red globules, with which the chapter begins, is by no means so accurate as it ought to have been. Leeuwenhoeck was one of the first discoverers of the existence of red globules in the blood. Soon after this discovery he observed globules also in chyle and milk, and thought that their diameter was only one-sixth of that of the red globules. Hence he was led to conjecture, that each of the red globules was composed of six of the globules which exist in the chyle. On adding volatile alkali to blood, he observed that the red globules were immediately broken down into smaller globules; and hence he conjectured, that the serum and

C

gluten

gluten consisted of globules. These conjectures were confirmed by a great number of additional experiments which led him to form this theory. The red globules are composed of six smaller globules, each of these of six smaller, and each of these perhaps of six still smaller; consequently a red globule is composed of 36, or perhaps even of 216, small globules.

Such was the theory of Leeuwenhoeck, as may be seen by consulting his works. Both the theory which our author ascribes to Leeuwenhoeck, and the improvement of it which he ascribes to Martine, belong to Boerhaave. That illustrious philosopher taught it long with applause, and published it in his *Institutes*. And though his hypothesis has proved erroneous, philosophy lies under considerable obligations to him for it; as it gave rise to a controversy which was not decided till a great deal of new light was thrown upon some of the most difficult parts of physiology. Boerhaave's theory was adopted by Martine, Winttingham, Helvetius, Noguez, Lieutaud, &c. and opposed by Lancisi, Brendel, Senac, and Haller, &c. by whose writings and experiments it was completely overturned. Our author has not mentioned one of these writers, nor has he produced a single argument against the theory of Boerhaave, or of Leeuwenhoeck, as he has thought proper to call it: For the arguments mentioned in the 71st page are not to the purpose, because they apply only to opinions which the philosophers who maintained that theory never held. Yet he has thought proper to treat an opinion with ridicule and contempt which he evidently did not understand, and to accuse (p. 69.) a man of attempting

ing to impose on his readers to whose writings he was a total stranger; a man who possessed a candour and zeal for truth which ought to entitle him to respect, and which must secure to him the esteem of every friend to virtue and science.

Our author, as it often happens with persons who enter keenly into a subject which they do not understand, is so zealous against this theory of Boerhaavè, that he has extended his resentment against the red globules themselves. They are not, he says, of that importance to the system which physicians have supposed. Many animals want them altogether; and in those which have them, *no likely use for them can be assigned*. Nay, what is worse than all this, the “disordered and miserable state of science, which continued for nearly a century, *arose from having observed too much these red particles.*” (P. 68.)

It might be urged in defence of the red particles, that they cannot justly be charged with having led philosophers into these blunders; and an instance will make the truth of this observation palpably evident. Let us suppose (since we are upon an anatomical subject), that an anatomist had been taught a little smattering of drawing; that he were very proud of that smattering; and that, in order to make his pupils admire his dexterity, he should fall a painting skulls, and drawing the figures of veins and arteries upon living men. We appeal to our author, if it would be fair to charge the art of drawing with all those absurdities into which such an anatomist happened to fall? We are not supposing that any anatomist was ever guilty of such childish absurdities; but allow-

ing, for illustration's sake, that such absurdities had happened, and that they had happened in Edinburgh, would that justify us in uttering a philippic against the art of drawing?

The hypothesis of Mr Hewson about the formation of red globules may be passed over, as the writers from whom our author has taken his refutations of it are known to every one.

Next follows our author's refutation of Mr Hunter's theory concerning the life of the blood. We have always considered what that ingenious philosopher has said on this subject as too vague and confused to convey any precise idea. It did not surprise us, therefore, when we found that Mr B. had written at least as confusedly as his predecessor, and that he had no accurate notion of the opinion which he had undertaken to refute. He talks of it as something in the highest degree absurd and ridiculous, and at the same time as entirely subversive of all our present physiological opinions. He tells us, that blood is in part a foreign body, and that it is contrary to all the laws of nature for the blood to be alive. Our readers will naturally ask for the proofs of this opinion. Here they are; "A fluid is a body whose particles often
 "are not homogeneous, have no stable connection
 "with each other, change their place by motion,
 "change their nature by chemical attractions and
 "new arrangements; a body which can have no perfect character, no permanent nature, no living
 "powers connected with it. But the definition of
 "a solid is the reverse of this: a solid among every
 "kind of metals, earths, or fossils, is recognized by
 "its

" its peculiar form and arrangement of parts ; and in
 " the animal body, the arrangement of particles gives
 " the permanent unchanging character of each part ;
 " and in the muscles, for example, or in the nerves,
 " where feeling and irritability chiefly reside, the
 " form and mechanism of the solid is in each most pe-
 " culiar, and is always the same.

" What is this blood that it should begin life and
 " support it, and distribute it through all the system?
 " Is it not a fluid which varies every hour, now rich-
 " er, now poorer, now loaded with salts, now drown-
 " ed in serum, now much, now sparingly supplied
 " with air, now darker coloured, now red, now fully
 " supplied with chyle, and now starved of its usual
 " supply ? Is it not lost in astonishing quantities in
 " hæmorrhagies, and drawn very freely from our
 " veins upon the slightest disease ? That such quali-
 " ties are consistent with life in the blood, is what I
 " cannot believe. But I can most easily imagine how
 " the system, having by successive operations, convert-
 " ed the food into chyle, the chyle into blood, and
 " fashioned the nutritious part of the blood into va-
 " rious solids ; that these new solids may partake of
 " the vitality of all the parts to which they are ap-
 " plied, and to which they have been assimilated by
 " so peculiar and so slow a process." P. p. 83, 84.

Thus our author has proved, incontrovertibly, that
 it is contrary to *all* the *laws* of nature for the blood
 to be alive. As we think his arguments excellent,
 we shall take the liberty of borrowing them for a
 little, in order to prove that it is *contrary to all the*
laws

laws of nature for a *solid* to be alive. They will answer exceedingly well, as our readers may see.

“ A *solid* is a body whose particles often are not homogeneous, have no stable connection with each other, change their place by motion, change their nature by chemical attractions and new arrangements ; a body which can have no perfect character, no permanent nature, no living powers connected with it. But the definition of a *fluid* is the reverse of this : a fluid, among every kind of metals, earths, or fossils, is recognised by its peculiar form and arrangement of parts ; and in the animal body, the arrangement of particles gives the permanent unchanging character of each part ; and in the muscles, for example, or in the nerves, where feeling and irritability chiefly reside, the form and mechanism of the fluid is in each most peculiar, and is always the same.”

“ What is a *muscle*, that it should begin life and support it, and distribute it through all the system ? Is it not a solid which varies every hour ; now richer, now poorer, now loaded with salts, now drowned in serum, now much, now sparingly, supplied with air, now darker coloured, now red, now fully supplied with chyle, and now starved of its usual supply ? Is it not lost in astonishing quantities in *amputations*, and cut very freely from our bodies in cases of *gangrene* and *cancer* ? That such qualities are consistent with life in the muscle is what I cannot believe. But I can most easily imagine, how the system having, by successive operations,

“ con-

“converted the food into chyle, the chyle into
 “blood, and fashioned the nutritious part of the
 “blood into various *fluids*, that these new *fluids* may
 “partake of the vitality of all the *solids* from which
 “they have been formed, and to which they have
 “been assimilated by so peculiar and so slow a pro-
 “cess.”

This is an admirable argument, and does our au-
 thor infinite honour. It settles the business com-
 pletely. No man will talk again of the life of the
 blood !

Our author next proceeds to the analysis of the
 blood. As his account is manifestly taken from the
 chemistry of Fourcroy and Chaptal, which are in the
 hands of every body, instead of following him mi-
 nutely, it will be sufficient to point out the mistakes
 into which he is continually falling, partly from not
 understanding his guides, and partly from venturing
 sometimes to wander from them.

In page 91 he says, “For the redness of the glo-
 “bules we know no *meaning* nor cause ;” yet he al-
 lows that they contain iron. “But,” says he, “the
 “cause which gives the oxyde of iron a red colour,
 “may give the blood a red colour.” True ; and in
 that case we may say with propriety, that we know
 not the cause of any thing whatever.

He tells us in page 93, that “the whole of the ani-
 “mal food which we eat is gluten, except the fat and
 “the earth of bones.” This is a mistake.—“That
 “flour contains *much* saccharine and extractive mat-
 “ter.” This is another mistake.—That the “mem-
 “branes, ligaments, tendons, periosteums, and all the
 “white

“white parts of the body, consist entirely of gluten,
 “and it is the business of cookery to boil them down
 “into this jelly.” Here are no less than seven mistakes in one sentence. He affirms, that “*no distinction* should be made between the *gluten* and *albumen* or *serum*; that *serum* exactly resembles the *white* of an *egg*; that the *tendinous* and *fleshy* parts of animals ought not to be distinguished (p. 96.); that serum contains *foreign* bodies, such as a *saccharine* or *extractive matter*, and some part of the *oxalic*, *malic*, or other vegetable acids (p. 97.); that there is *no difference* between the *red globules*, the *gluten*, and the *serum*; that such distinctions are *ignorant* and *unmeaning*; and that the *balitus* of the blood is merely *water alone*, having a slightly *urinous* smell from its connection with the blood” (p. 98.) All these assertions are not only directly contrary to truth, but most of them are so completely ridiculous, that they could not have been maintained by any person who had the smallest knowledge of the subject.

He tells us farther, that all our solids and fluids can be resolved into gluten; that, “bating the various proportions of the water which dilutes the serum and the red globules (whose proportion to the fluids cannot be named it is so small), and some saccharine or extractive matter which is in the serum of the blood—what is there but gluten in all the animal system? Serum, coagulum, flesh, tendons, ligaments, bones, all are composed of it; and when gluten is thus united to the solids, forming with them one individual body, it acquires new powers, and is indeed alive.” (P. 99.)

This

This is one of the most complete instances of absurdity and nonsense that can well be conceived. How a man should have thought of writing on a subject of which he was totally ignorant, and of attempting to reason on chemistry without knowing the very terms of the science, is totally inconceivable.

He tells us farther, that the analysis of the blood *contains almost* the analysis of *all* the humours and secretions of the body; that *urine* very nearly resembles *serum*; that *sweat* is but a *serum* loaded with *salts*; that *saliva* differs but little from *serum*; that *milk* perfectly resembles *serum*, since mixing *serum* with *water* produces a *milky fluid*, that is, a fluid which gathers *cream* on the top; that the *water of dropsies* is *pure serum*; and that the *mucus* of hollow passages is little else than inspissated *serum* (p. 100.).—Our author might have added, with equal propriety and equal justice, the bones also are pure serum, the muscles and nerves are pure serum, the whole body is composed of serum; fluid, solid, and bony serum.

By the bye, though our author began with telling us, that the red globules are of no great importance in the system, he has here shewn that the whole system is composed of red globules: for the whole solids and fluids may be resolved into gluten or serum; and there is *no difference* between gluten, serum, and red globules.

The author now comes to an explanation of the function of respiration on chemical principles; and we have seen already how well qualified he is for the task. He begins with an account of the present state of chemistry. “The *simplicity* of the facts in che-

“mistry, the *correctness* of the reasoning, the *grandeur* which now the whole science assumes, is *very pleasing*, and makes us not without hope that in this science *all* others, and *ours* in an especial manner, may be improved.” (P. 101.)—His readers might be at a loss to know whether it is the *simplicity*, *correctness*, or *grandeur* of chemistry, or altogether being *very pleasing*, which leads our author to form these hopes; but he tells us himself, at the end of the sentence, “For the action of vessels *will do* much in *forming* and *changing* our fluids; *all* the rest is chemistry alone.” This is a very satisfactory reason, and not the less so that it is totally destitute of meaning.

“The older chemists were coarse in their methods, bold in their conjectures, in theory easily satisfied with any thing which others would receive. They condescended to repeat incessantly the same unvarying process over each article of the *materia medica*; and among hundreds of medicinal plants which they had thus analysed, they could find no variety of principles, nor any other variety of parts and names than those of phlegm, and oil, and alkali, and acid, and sulphur, and coal.” (P. p. 101, 102.)

Reader, these older chemists were Sir Isaac Newton, Boyle, Boerhaave, Hales, Stahl, Homberg, Geoffroy, &c. the founders of the science; entitled to the respect and veneration of every chemist, and many of them the authors of discoveries which have been the glory of their country and of their age.

“The

“ The older chemists thought that they had arrived
 “ at the pure elements, while they were working
 “ grossly among the grosser parts of bodies. When
 “ they had converted bodies into air, they thought
 “ them annihilated. When they thus stopped at airs,
 “ they stopped where only their analysis became in-
 “ teresting or simple ; stopping where they stopped,
 “ among their oils and sulphurs, made their science a
 “ mere rhapsody of words. Philosophy they confi-
 “ dered so little, as not to know that the lightest air
 “ is really a heavy body, and that with weight and
 “ substance other properties must be presumed.”
 (P. 102.)

Who these older chemists were to whom the au-
 thor alludes, it would be impossible for Oedipus him-
 self to guess. Nobody that deserves the name of che-
 mist preceded Galileo and Torricelli ; and since their
 time, who has been ignorant of the weight of air ?
 Nay, farther, Mr Boyle, one of these older chemists,
 is the person to whom philosophers are indebted for a
 great part of their knowledge of the properties of air ;
 and Dr Hales, another of them, is the person who
 laid open the path of pneumatic chemistry.

“ Modern chemistry begins by assuring us, that
 “ *these airs* are often the *densest* bodies in the *rarest*
 “ forms.” (P. 102.) This chemistry must be *very*
modern, indeed which begins with such assertions.
 What is a *dense* body in a *rare* form ? We might as
 well talk of a *light* body in a *heavy* form, or a *white*
 body in a *black* form, or a *cold* body in a *hot* form.
 The author has had no distinct conception of what
 he was writing.—“ That airs are as material, as ma-

“ *nifest to the senses*, as the dense bodies from whence
 “ they are produced.” (P. 102.) If this be true,
sight must be excluded from the senses.—“ That it is
 “ *heat* ALONE that converts any substance into the ae-
 “ rial form: That some bodies require for their flui-
 “ dity merely the *heat of the atmosphere*” (*This is an*
expression which has no distinct meaning): “ That others
 “ require some *new* principle to be added, in order to
 “ give them the gaseous or aerial form.” (P. 103.)
 Do they indeed? Then it is not true that *heat alone*
 converts *any* substance into the aerial form.—“ That
 “ *all* aerial fluids arise, or must be presumed to arise,
 “ from some solid basis, which solid basis is dilated by
 “ *heat* into an air.” (P. 103.) So it is *true*, after all,
 that *heat alone* converts *any* body into an aerial form!
 Reader, whichever of the two opinions proves true,
 our author has adopted it. You see the unspeakable
 advantage of this way of writing!

“ These airs can be alternately combined with a
 “ body, and abstracted again, *adding* or subtracting
 “ from its *weight* and *chemical* properties, not only in
 “ a *perceptible* but in a wonderful degree; so that these
 “ *abstractions* and *combinations* constitute some of the
 “ most *general* and *important* facts.” (P. 103.) This
 is wonderful indeed! and he that can decypher the
 meaning must have more ingenuity than we can pre-
 tend to. It is very beautiful for all that.—“ When
 “ the old chemists then neglected to examine these
 “ airs, they refrained from examining the *last elements*
 “ of bodies at the very moment in which they came
 “ within their power.” (P. 103.) *The last elements*
of bodies! This is a very important discovery; and
 we

we take the liberty to thank our author for it, in the name of the whole body of chemists: for we can assure our readers that it belongs entirely to our author; no *modern* chemist, as far as we know, having ever dreamed of it before.

“ The older chemists observed, that when they burnt an inflammable body, the surrounding air was contaminated, the *substance itself was ANNIHILATED*, nothing remained of its former existence but “foul air.” (P. 104.) What older chemists made the observation, that inflammable bodies, by being burnt, were *annihilated*, we pretend not to divine.— “ They supposed that this inflammable body consisted of a pure inflammable principle, which was the substance which spoiled the air, lessening its bulk, and making it unfit for supporting any longer either combustion or animal life.” (P. 104.) The *older* chemists who formed this theory, which was a very important improvement of the theory of Stahl, were Dr Rutherford and Dr Priestley. Why our author classed these ingenious philosophers among the *older* chemists, let our readers determine; he evidently classes himself among the *younger* chemists, a place to which he is *eminently* entitled, if he does not actually stand at the very bottom of the list.

“ Modern chemistry has explained how *all* these phlogistic processes (*combustion, calcination of metals, respiration*) depend, not on the abstraction of phlogiston, but on the addition of a new principle; that they *all* arise from *one* positive power; that the *same principle* gives *life* to fuel, *beaviness* (and other effects of calcination) to metals, *acidity* to
“ acids,

“acids, and *redness* to the blood. These are *all* per-
 “formed by *one* power; they are *all essentially ONE*
 “process; they are *all* effected by the communica-
 “tion of *one sole* principle, viz. the basis of pure air.”

(P. 107.) Happening to turn over to page 123, we
 observed this passage: “*Burning* and *rusting* are ve-
 “ry *different*, and so *combustion* and *respiration* are.”

We make no doubt that this passage, which is a flat
 and unqualified contradiction of the paragraph just
 quoted, will seem strange to most of our readers; but
 they will please to observe, that our author's argu-
 ment would have been *absurd* in page 123, if he had
 supposed these processes *the same*, and his explanation
 would have been *absurd* in page 107 if he had suppo-
 sed them *different*.

This is a species of argumentation which we would
 recommend to the attention of our readers. It is but
 very little known. We do not recollect to have seen
 it taken notice of in any system of logic, though we
 have examined a great number on purpose. Nay,
 what is still more, the professor of logic in our uni-
 versity does not exhibit a single specimen of it in the
 whole of his lectures. Mr Bell is a perfect master of
 it, and the rules for using it might easily be deduced
 from his writings. We humbly propose, therefore,
 in honour of our author, who may in some measure
 be considered as its inventor, to give it the name of
 JOHN BELLATION.

Every body knows the importance of the signs plus
 and minus in algebra, and how by their assistance ma-
 thematicians are enabled to extricate themselves out
 of the greatest difficulties. Johnbellation will be equal-
 ly

ly useful in the other sciences. Indeed it is founded upon the same principles with the use of the signs plus and minus in algebra, and is merely an extension of them. Suppose we have any proposition, for instance this, *combustion and respiration are the same*: A writer, ignorant of johnbellation, if he wanted to prove that heat is not evolved during respiration, would be at a loss how to proceed, or how the proposition could help him out; but the knowledge of johnbellation would remove all his difficulties at once: for it proceeds upon this postulate, that every proposition may be taken either *negatively* or *positively*. Consequently, if it be true that combustion and respiration *are the same*, it is true also, according to the principles of johnbellation, that combustion and respiration *are not the same*. Here then we have our choice of two arguments; one or other of which must always be to our purpose. Consequently, johnbellation gives us this unspeakable advantage, that it enables us to prove any thing we please by arguments perfectly irrefragable and invulnerable.

The principles of johnbellation might easily be deduced from the writings of our author; and we may perhaps at some future period favour the world with a treatise on the subject; unless indeed, which is a thing rather to be wished, our author anticipate us, by publishing himself the principles of his art. In the mean time, for the satisfaction of our readers, we shall inform them, that johnbellation is divided into several branches, each of which has its peculiar rules and its peculiar advantages.

I. The

L The first species of johnbellation is *negative* and *positive* johnbellation, or johnbellation properly so called. This is the species which we have described above. As example is in all cases better than precept, instead of laying down rules for using it, we shall produce a beautiful example or two from that volume of our author's valuable writings which we are at present considering.

1. "Not upon *any animal*, but in the *human* body." (P. 123.)

2. "Nature has appointed in every breathing creature *two hearts*." (P. 4.)—"The frog, the newt, the toad, have *one single and beautiful* heart." (P. 5.)

3. "Of an hundred measures of atmospheric air, we find twenty-seven only to consist of vital or pure air; seventy-two consist of azotic air as it is called, fatal to animal life; and one measure only is fixed air, which is also an unrespirable air. But of these twenty-seven parts of pure air, *seventeen* parts only are affected by respiration; so that in respiration we use much less than a fifth part even of the small quantity of air which we take in at each breath." (P. 127.)

"Our atmosphere is so constituted as to hold but a fourth part of vital air, and of that small proportion *one half* only is used in the lungs." (P. 128.)

4. "We may fairly begin our next general fact under the title of the oxydation or *oxygenation* of the blood." (P. 113.)—"We call this process not the *oxygenation*, but the *oxydation* of the blood." (P.

117.)—"It is *not* a fair nor permanent oxydation." (P. 121.)

5. "The ductus venosus enters the largest of the *hepatic veins*." (P. 173.)—"The ductus venosus enters the *heart*." (P. 174.)

6. "The stimulant power of oxygene is most of all apparent when we force a living creature to breathe nothing but the purest air; for oxygenated or vital air makes this process too rapid; the pulse rises, the eyes become red and prominent, the creature seems drunk with the new stimulus, too great for its system. The *universal heat of its body is greatly increased*, the eyes are turgid and red, and at last a sweat breaks forth all over it; and when dead, the lungs (it is said) are mortified or inflamed." (P. 116.)

"The next effect of oxygene is said to be the communicating of heat to the lungs. But I suspect, that if the small quantity of oxygen which can enter by the lungs *do communicate heat*, it must be not to the *lungs*, nor to the *blood*, but to the whole body, through the medium of the blood. There are some who pretend to say, that when they draw in vital air, *they feel a genial warmth* in the breast, diffusing itself over all the body; but it is easy to feel in this way, or any way, when a favourite doctrine is at stake, while those who know nothing about doctrines breathe the vital air *without any peculiar feeling* which they can explain." (P. 117.)

II. The next species of johnbellation belongs exclusively to our author: he uses it upon many occasions, and with great address. It may be called *double*

johnbellation. It consists in making use of arguments which would apply equally well to *both* sides of the question, and which of course serve at one and the same time to *prove* any thing and to *disprove* it. The best instance of the double johnbellation that occurs anywhere is a passage formerly quoted in p. 20, 21. It is an argument against the possibility of life existing in fluids, and consequently a proof that it exists only in solids. If we substitute *solids* for *fluids*, this *admirable* argument will prove equally well that *solids* cannot possibly be *alive*, and consequently that all living bodies *must* be *fluid*.

III. The third species of johnbellation may be called *universal johnbellation*. It is founded on this axiom: Whatever mistake has been committed by any *one* philosopher who has written upon any particular science, has been committed by *all those* who have written or studied that science. Our readers will easily see, that this is not the least important branch of johnbellation; and they can easily conceive how immensely useful it must be to those who wish to be very sagacious, and very deeply versed in science. Our author is fully sensible of its great importance, and has therefore very often called it to his assistance.

By means of it, he has made John Hunter's mistake about the diaphragm of birds the mistake of the whole *mob* (to use our author's elegant phrase) of anatomists and physiologists. Accordingly he has very obligingly put the *world right* in this particular.

By means of universal johnbellation, the mistake of some of the older physiologists about the use of respiration

ration is made the opinion of all the physiologists of the present day; and our author is obliging enough to put the *world to rights* also in that particular.

By it also the hypothesis of some physiologists about the red globules is ascribed to the whole of physiologists, even to those who refuted them; and our author is obliging enough to put the world to rights in this particular again.

IV. The fourth and last species of johnbellation may be called *individual johnbellation*. It is the reverse of the former, and is founded on this axiom: Every discovery which has been made, and every idea which has been started, by *any person* who has written on any particular science, *may be claimed and appropriated by any other individual* who is engaged in the same science. This species of johnbellation has been of infinite importance in the hands of our author. The individual of whom he has made choice, and in whom he has concentrated all the discoveries and thoughts of others in physiology and anatomy, is, as was most fit and proper, *his illustrious self*.

By this species of johnbellation, the description of the valve of Eustachius, given by our author, is *his own*; the account of the irritability of the heart is *his own*; the refutation of Hewson and Hunter is *his own*; the analysis of the blood is *his own*; the experiments made concerning respiration are *his own*; the account of the respiration of birds is *his own*; the account of the respiration of amphibia is *his own*; the account of the respiration of fishes is *his own*; the anatomy of insects is *his own*; and the account of

the circulation of the foetus, and the use of the placenta, are all *his*, and *his alone*.

Such are the unspeakable advantages which result from the judicious use of individual johnbellation. Our author without it might have passed for a compiler or a collector; but by his skill in individual johnbellation, together with a judicious use of universal johnbellation, and the other two species of johnbellation formerly described, he has raised himself to the rank of an original and profound writer; and has demonstrated to the world, that he is possessed of more knowledge and more sagacity than all the anatomists and physiologists who have preceded him. Reader, if you are possessed of a spark of ambition, spend your days and your nights in the study of johnbellation; and endeavour, by a judicious use of that noble art, to become one day as *great* a man as our celebrated and illustrious author.

But we return from this digression, which we hope our readers will forgive, on account of the great importance of the information which it contains.

“ Could we have supposed that it (the atmosphere) “ was the cause, not merely of life in all living creatures, but almost the cause of all the properties that “ reside in the most solid forms?” (P. 107.) We at least could never have conceived that it was the *cause* of life, because we do not *believe* that it is the cause of life. And, by the bye, is it not singular that *no fluid* can have life, as we have seen our author formerly prove, and yet that a fluid should be the cause, and the only cause, of life? That *our atmosphere should be almost the cause of all the properties that reside in the*
most

most solid forms, we never could have conceived, nor can we conceive it at this moment, because we do not understand the meaning of it.

“Combustion is a process which consists in the rapid assumption of the basis of pure air, and the consequent *conversion* of the burning body into an air endowed with peculiar qualities and powers.” (P. 109.) Every tyro knows, that it is not true that all bodies are converted by burning into an air. Why then does our author affirm that they are?

“Must it not be presumed, that the principle which gives an increase of weight, and such singular properties to metals, have very interesting effects on the blood?” (P. 110.) Surely; and must not acids, which give an *increase of weight*, and *such singular properties* to metals, have very interesting effects on the blood?

“From this principle (oxygen) *all acids* are formed.” (P. 110.) This has never yet been proved, but we shall not dispute about it.—“And as *oxyd* is the Greek name for acid.” *Oxyd* is not a Greek word at all, and if it were, the Greeks had no word to signify *acids*; for the best reason in the world, they were not acquainted with them. They had indeed a name for vinegar, and an adjective signifying *four*, from which *oxyd* has by a slight change been obtained. We would not have mentioned this blunder at all, had it not been for the eagerness which our author displays on all occasions to show his learning, by explaining the meaning of words borrowed from the learned languages. He is even more unfortunate on other occasions than we have found him at present, sometimes mistaking

staking the meaning of the word altogether, and even the language from which it is taken.

In page 111, we are told that the ancients mistook *azotic air* for *their phlogiston*. We are utterly at a loss what meaning to affix to the term *ancient* in this passage. We know of no ancient or modern who made this mistake. The phlogistic theory was introduced into chemistry since the beginning of the present century; consequently *the ancient* to whom our author alludes must have lived since the year 1700. Nay, farther, phlogistic air was unknown before 1770, and consequently this ancient opinion cannot be 30 years old.

“In burning arsenic we have combustion, calcination, and generation of acid, all in one process; the product being named indifferently *oxyd of arsenic*, or *white calx of arsenic*.” (P. 111.) If this be the product, where is the generation of an acid? The *oxyd of arsenic* is not an *acid*.

“This principle (oxygen), which bestows weight and causticity on metals, acidity on acid bases, and new properties on all it touches, *must* make similar, or at least important, changes on the blood, converting it into an oxyd or subacid; and we may fairly begin our next general fact under the title of the oxydation or oxygenation of the blood.” P. 113.

Our author has now come to the effect of respiration on the blood; and the passage just quoted contains the proofs of that effect. It is therefore of importance. Let us reduce it to the form of a syllogism. Oxygen bestows *weight* and *causticity* on metals (*does it?*), acidity on acid bases, and new properties

ties

ties on *all* (what ALL?) it touches; therefore it *MUST* make *similar*, or at least *important, changes* on the blood, converting it into an *oxyd or subacid*; therefore we may fairly begin our next general fact under the title of the OXYDATION or OXYGENATION of the blood. Such is our author's argument for the *oxydation or oxygenation* of the blood. It is an admirable specimen of double johnbellation: indeed it is as perfect a double johnbellation as can well be conceived; for it would apply with equal facility, and with equal effect, *mutatis mutandis*, to prove or to disprove any thing whatever.

Our readers, all of them at least who are not total strangers to chemistry and physiology, know, that concerning the changes produced upon the blood by respiration there are two opinions: One, that no oxygen enters into the blood, but that the change of venous into arterial blood is owing to the extrication of a quantity of hydrogen and carbon from it in the lungs: that these bodies combine with part of the oxygen of the air inspired, and form with it water and carbonic acid—The other, that oxygen actually enters into the blood in the lungs, combines with it during the circulation, and is again extricated when it returns to the lungs.

The first of these opinions has been adopted by Crawford, Lavoisier, Gren, Seguin, &c. and supported by the most accurate and expensive experiments which have been made on the subject. The latter opinion has been adopted by La Grange, La Place, Hassenfratz, Girtanner, &c. and has also been supported by very ingenious experiments. The difficulty of examining

mining this subject with accuracy is exceedingly great; and we do not think that all the experiments which have been made, numerous as they are, entitle any person to consider the question as decided. Accordingly, those physiologists who have paid the greatest attention to the subject, and who of course are best qualified to judge of it, consider it as still *sub judice*.

Our author, however, decides upon it very peremptorily; and we have seen already the force of the proofs, by which the reasonings and experiments of Crawford, Lavoisier, and Gren, have been refuted and laid aside.

Perhaps what he says in page 115 may be considered as additional proofs, and indeed they are very pretty specimens of *double jobnbellation*; but we do not think them equal to the passage we last quoted.

After being thus completely convinced by our author of the *oxydation or oxygenation* of the blood, we happened to turn over to pages 117 and 121, and the following passages struck us: "We call this process, "NOT the *oxygenation*, but the *oxydation* of the blood, "because we are conscious that it is an imperfect "process—it is so imperfect, that we put it into the "lowest point of *saturation*, and call it (what it?) an "oxyd or *imperfect acid*; and how far it may be be- "low the denomination even of an *oxyd* we do not "know." (P. 117.) "It is *not* a fair nor permanent "oxydation—the oxygen seems but *slightly* attached "to the blood; it is not so much *united* with the blood "as conveyed by it." (P. 121.)

We

We look upon these passages as exceedingly beautiful specimens of *negative* and *positive* johnbellation. We regretted only that our author had left us at a loss to know whether by respiration the blood be *oxygenated* or *oxydated*, or *converted* into an *oxyd* or *sub-acid*, or whether *any change* be produced upon it at all in the lungs. But on turning back to the 113th page, we have been fortunate enough to find our doubts completely removed. "Nature," he says, "disregarding all occasional supplies, has appointed one great organ for the OXYGENATION of the blood, viz. the lungs." This was certainly very kind in Nature; and the more so, as we consider ourselves as in some measure beholden to her for the removal of our doubts.

"When we expose blood to oxygen gas, the *purest* of all airs (*is not azotic, or carbonic acid, or hydrogen gas, equally pure?*), it grows extremely florid; and whenever it changes its colour, it is by absorbing oxygene or pure air; for it reduces in the same proportion (*with what?*) the quantity of air." (P. 114.)—These assertions are directly contrary to the experiments of Seguin and Gren, and we may add, too, of Lavoisier, Priestley, and Crawford. We would wish therefore to know upon what authority they are founded.

The next paragraph contains several experiments of Priestley and Menzies, a good deal disfigured: and by the rules of *individual johnbellation*, to which our author on all occasions adheres very closely, the names of the authors are omitted, and our author speaks in the *first* person.

Our author now comes to the consideration of the *beat* of the blood. He had affirmed in page 108, that heat is produced during *respiration*; and he had told us in page 116, that when an animal breathes oxygen gas, *the universal beat* of its body is *greatly increased*. But as his object in this place is to refute the theory of Dr Crawford, he has very properly called in negative and positive johnbellation to his aid; and accordingly he begins the subject with this observation: "There are *some* who *pretend* to say, that when they "draw in vital air, they feel a *genial warmth* in the "breast, diffusing itself over all the body. But it is "easy to *feel* in *this* way, or *any* way, when a favourite doctrine is at stake, while those who *know nothing about doctrines* breathe vital air *without any* "peculiar feeling which they can explain."

Having thus happily begun the subject in due form, he proceeds to refute Dr Crawford's theory by the following arguments, which we shall take the liberty of examining.

1. The oxydation of the blood *out* of the body produces *no beat*, consequently it ought to produce *no beat in the body*. (See p. 117.)

We are somewhat at a loss to discover the meaning of this argument. Does it suppose that the *same change* takes place in the blood when exposed to the air out of the body as when in the lungs? If so, we should take it kind if our author would *produce* the *proofs* which led him to form such a conclusion. They would remove all the difficulties which have hitherto perplexed the subject of respiration. If the author has no such proofs, as we suspect strongly from his not having

having produced them, his argument is of *no* weight; because there are a thousand chances to one that the blood does not undergo the very same changes when out of the body as when in the body. Nay, farther, we would advise our author, before he affirms so confidently that *no heat* is evolved by exposing the blood to oxygen gas, to re-examine all the circumstances. If he does so fairly and skilfully, we shall venture to predict that he will not again make such confident assertions.

2. Our author's second argument is, that "to suppose but for a moment that all the heat which warms the whole body emanates from the lungs, were a gross error in philosophy. It were to suppose an accumulation of heat in the lungs equal to this vast effect of heating the whole body." (P. 118.)

This argument is still worse, if possible, than the former. It goes upon the supposition that Dr Crawford taught, that all the heat necessary to continue the temperature of the body is evolved in the lungs during respiration, and is from thence distributed to the whole body, precisely as if a fire or a candle were placed in the lungs. Such an opinion would indeed be unphilosophical, but it is very far from being Dr Crawford's opinion; and therefore its being unphilosophical, is no argument whatever against the theory of that very ingenious philosopher.

Dr Crawford's theory is this: The capacity of arterial blood for heat is greater than that of venous blood, yet its temperature is the same; consequently it *must* contain more heat. Venous blood is converted into arterial blood in the lungs; and since its tem-

perature is not diminished, it *must* receive heat in the lungs. Arterial blood is converted into venous blood during its circulation; therefore it *must* gradually give out heat during its circulation. It is this gradual evolution of heat that maintains the temperature of the body.—Such is an abstract of the theory of Dr Crawford; we do not say that it belongs to him originally, but we are indebted to his labours for the facts by which it is supported. These facts, allowing their truth, render it invulnerable; and it is not by misrepresenting it and railing at it, but by examining the facts which form its basis, that it must be either established or refuted,

One of these facts our author has considered in the following terms: “ Dr Crawford was extremely anxious to prove, that in proportion as air was changed by respiration, it gave out its heat to the blood; he also wished to put respiration and combustion on one level; and by this second thought he forgot entirely what he first had in mind to prove. Accordingly, having inclosed a Guinea-pig in pure air, and under water, he found that the air which it had respired communicated nearly the same heat to water that burning the same quantity of air should have done: by which he proved much more than he intended; he proves plainly by this, that all the heat which respiration can possibly generate is by the fixed air carried from the lungs, and he forgot to reserve any for going into the blood.” (P. p. 120, 121.)

Now so different is our opinion concerning this experiment of Dr Crawford from that of our author,
that

that we consider it as a very strong argument in favour of Dr Crawford's theory. The temperature of hot blooded animals is considerably above that of the surrounding atmosphere; therefore they must be continually giving out heat to the surrounding bodies. But their temperature is constant; therefore they are continually receiving a quantity of heat just equal to what they are giving out. In Dr Crawford's experiment, the water must have been heated partly by the warm air which the animal expired, and partly by the heat which was continually passing out of all parts of its body. Now as the temperature of the animal would continue the same, it must have been constantly receiving a quantity of heat equal to that which it was losing. If therefore the water was raised exactly to the degree of heat to which it would have been raised by the combustion of the same quantity of oxygen gas which was consumed in respiration, it follows, that the oxygen actually parted with all the heat lost, and that therefore a quantity of heat exactly equal to what the animal *lost* during the experiment must have been furnished it by respiration; which is the very thing that Dr Crawford wanted to prove.

This is the only experiment of Dr Crawford which our author has thought proper to examine. But he tells us that these experiments were very ill made; that they proceeded upon very fantastical and absurd laws; that they were much fitter for a magician than a philosopher to undertake; that the intricacies of Dr Crawford's theory are its beauties; that it is a hypothesis illustrated by experiments, which have no other tendency than to make it look well in the face,
and

and which are made with such affectation of niceness as is completely ludicrous ; that he begins his doctrine with a *petitio principii* ; and that his main experiment is wrong*.

Such is the ungenerous and unmanly language in which this writer chuses to speak of the labours of Dr Crawford ; one of the most amiable and ingenious men whom the present century has seen. His theory is so completely misrepresented, and the small number of his experiments which Mr Bell has thought proper to mention are so wretchedly ill stated, that it is not possible for us to suppose that Mr Bell has ever read Dr Crawford's book.

It is not worth while to examine our author's explanation of the production of animal heat. His opinion, if he can be said to have any opinion at all, coincides with the hypothesis of La Grange and La Place, as illustrated by Hassenfratz and Girtanner. In his attempts to establish this opinion, he contradicts every thing which he had said in his refutation of Dr Crawford, and adopts the very same first principle which he had a few pages before vilified as a *petitio principii*. For he lays it down as a law of nature, that all bodies, on passing from a fluid to a solid form, give out heat ; yet in page 119 he ridicules Dr Crawford for supposing that flesh, rye, barley, &c. contain less heat than blood.

In page 122 he affirms, that a part of the oxygen gas inspired combines in the lungs with inflammable
air.

* This is the experiment which we have just considered.

air. Where *this air* comes from *we* cannot conceive. In page 125 he tells us, that the *acidum pingue* is the same with the acid of fat. Every body knows that the *acidum pingue* was a hypothetic acid of Meyer, which has no connection whatever with the acid of fat.

The last section of this long chapter is entitled, *Of the Respiration of Plants*. It contains the following propositions: 1. Water is compounded of oxygen and hydrogen. 2. The structure of plants is perfectly simple. 3. Plants absorb and decompose water. What these facts have to do with the respiration of plants, we cannot conceive. By the bye, our author's proof of his second proposition, that the structure of plants is perfectly simple, is an excellent double johnbellation, and might be employed with equal success to prove *the perfect simplicity* of the structure of animal bodies, or to prove that the structure of vegetables is exceedingly *complex*.

Our remarks upon this chapter have been rather long, and probably our readers will be fatigued; but the following pretty little story, which we have selected with great care and after infinite research, will, we doubt not, recover them entirely.

THE LOOKING GLASS.

A bear of shag and manners rough,
At climbing trees expert enough;
For, dext'rously, and safe from harm,
Year after year he robb'd the swarm.
Thus thriving on industrious toil,
He glory'd in his pilfer'd spoil.

This

This trick so swell'd him with conceit,

He thought no enterprize too great.

Alike in sciences and arts,

He boasted universal parts ;

Pragmatic, busy, bustling, bold,

His arrogance was uncontrol'd :

And thus he made his party good,

And grew dictator of the Wood.

The beasts, with admiration, stare,

And think him a prodigious bear.

REMARKS

The Looking Glass.

A bear of bag and mumping tongue.

At climbing trees expert enough ;

For, dex'terous, and his front hairy,

Year after year he robb'd the farm.

Thus thriving on industrious toil,

He glory'd in his effort's toil.

THIS

REMARKS

ON

Mr JOHN BELL'S ANATOMY

OF THE

HEART AND ARTERIES.

ON CHAP. III.

WE come now to the third chapter, which is intitled OF RESPIRATION, and which may be considered as the most perfect specimen of *universal* and *individual jobnbellation* in any language. We therefore recommend it most earnestly to the careful study of every ingenuous young man who is ambitious to excel in that noble and important art.

The division which our author has adopted in this chapter belonged originally to a celebrated French writer whom he has not mentioned, as far as we recollect, in his whole book.

"It is now full time," says he, "to *correct* many "*mistakes* into which *modern* as well as *ancient* authors "have wandered from want of general principles, "and from want of anatomical knowledge. I shall "endeavour to make this chapter *interesting* and "*short*." (P. 134.)

After this excellent commencement, our author enters upon a refutation of those anatomists who thought that the lungs are possessed of a muscular power.

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Malpighi,

Malpighi, Thurston, Swammerdam, &c. had trodden the same ground before him; and all anatomists have long known that the lungs possess no such power. Our author has with great propriety omitted to mention all this, and has introduced no anatomist or physiologist, except those individuals who entertain erroneous opinions concerning the nature of the lungs. He is therefore to be reckoned the *first* person who explained the *real structure* of the lungs. This is one great mistake which our author has corrected by virtue of individual and universal johnbellation.

The only unlucky circumstance is, his observing that man, and all animals that *breathe by a diaphragm*, have heavy lungs of a strong *fleshy* texture (p. 137.) This would lead one to suppose our author an advocate for the opinion which he had just so *ably* and so *modestly* refuted; for if the lungs be of a *fleshy texture*, they *must* be *muscular*. But this small slip of our author vanishes into nothing when contrasted with the admirable individual johnbellation which follows immediately after, by means of which he has made himself the *first* person who has given an accurate account of the diaphragm, and of the manner in which respiration is performed in man.

“Forsaking for a moment *authority* and minute anatomy,” says he, “let us explain it in the shortest and most intelligible way.” (P. 137.)

The greatest part of the second section is employed in refuting a singular opinion of John Hunter, viz. that fowls breathe by the help of a diaphragm. This opinion our author has ascribed to all anatomists and physiologists. “Until I set this point to rights,” says he

he, "*my* arrangement" (that is, *my* arrangement by virtue of individual johnbellation) "is good for no-thing." (P. 139.)

After our author has corrected this second mistake into which *all writers ancient as well as modern* have fallen, he proceeds to give an account of the respiration of birds; and, by virtue of individual johnbellation, to which our author, as usual, has had recourse, this account is entirely a *new account*, and was *never* before conceived by any writer either ancient or modern. The fact, however, is, that the same account had been given by Swammerdam and many other philosophers; and that the respiration of birds was familiar to all anatomists and physiologists who were not (to use a johnbellation) totally ignorant of anatomy and physiology.

In the third section our author gives an account of the respiration of amphibia, or of those animals *which are said in the first chapter not to breathe at all*. This account he has also rendered his own by individual johnbellation. A hundred years ago it belonged to Swammerdam and Malpighi. We suspect, however, that our author has only paid attention to these amphibia upon paper, and not very much even there. He has selected the frog as an instance of the respiration of these animals, and has placed at the beginning of his chapter of respiration a drawing which, he says, represents the frog's mouth. But of the frog of this country it certainly is no accurate representation. "At (*a*)" he says, "is seen its tongue of *prodigious length*; it is *binged*, not like the tongue of any other creature, far back in the

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" mouth,

“ mouth, but is fixed in the chin to increase its length ;
 “ at the further end it is forked. We see it launch-
 “ ing out this *monstrous tongue in catching flies ; per-*
 “ *haps also with this it rakes mud.*” (P. 146.)

This sublime description cannot surely apply to our British frog ! He is at great pains to inform us, that the frog, which *launches out this monstrous tongue*, never opens its mouth, and that it always keeps its mouth under water ; facts which every school-boy (to use a Scottishism) knows to be false ; and thinks it a very peculiar property in that animal that it breathes through its nostrils. Now we always thought that this had been the case with man and all other animals that have nostrils. Though this section is intitled *Of the Respiration of AMPHIBIÆ*, our author insists that these animals are *not amphibia* ; that their being able to live for a considerable time under water is no proof that they are, because they will live as long without their heart or their head. Now frogs may be made to live for weeks, and even for months, under water ; but who ever heard of a frog living for weeks or for months without its head or its heart ? He tells us farther, that it is the nature of the lungs of these animals to *oxygenate* but a small quantity of blood, and that they have not the same occasion for respiration. This may be the case ; and if so, they ought to be *amphibia*. But our author's proof is not to the purpose. He argues from the small quantity of blood which is sent to the lungs at a time ; but Dr Hales has shewn, that the blood circulates in the lungs of a frog 43 times faster than in the muscles ; consequently, supposing that only 1-43d part of the blood propelled by the heart

heart went to the lungs, all the blood might pass through the lungs for every time that it circulated through the body.

The whole of the fourth section, in which our author treats of the respiration of fishes, belonged originally to Boyle, Swammerdam, Willis, and Monro. Our author, however, has appropriated it to himself by individual johnbellation.

But the most singular section in the whole chapter is the fifth, on the respiration of insects. The whole of it is taken very faithfully from Swammerdam, with the precaution of altering the language and the arrangement. It is illustrated with nine or ten figures, all of which except two are taken from Swammerdam. Yet our author has adhered so strictly to the rules of individual johnbellation, that he not only never mentions Swammerdam's name, but expresses his astonishment that these ideas never occurred to any writer before himself. "I only mention difficulties," says he, "which it is surprising that others have not declared and investigated."

We only regret that our author was totally ignorant of the numerous discoveries which have been made in this branch of natural history since the days of Swammerdam; for then he would have been able to explain those difficulties which *nobody has declared and investigated*; and he might have elucidated the function of respiration by the application of several very important facts, which, if followed out, might lead to a method of deciding at least a part of the difficult question concerning the changes produced upon the blood in the lungs.

We

We are sorry, too, that our author has not studied Swammerdam with greater care; he would not then have talked of the *air vessels* of *snails*. We are still more sorry to see him affirm, that insects destroy proportionally more air than large animals, and that many insects live best in the *foulest* air; because these assertions entirely destroy the effect of the beautiful johnbellation contained in this section, by pointing out too clearly to the reader that the author is writing about a subject of which he is *totally ignorant*. Had it not been for these unlucky slips, and one or two more; such as "bags resembling the *alga marina* or *sea-weed* in shape," and "*rigid* tubes like a *flexible* catheter," we would have considered this section as the boldest and most excellent individual johnbellation in the book, and would accordingly have recommended it to the careful study and imitation of the aspiring reader.

The author ought to have concluded this chapter with the following

PROCLAMATION.

We hereby prohibit all our readers and pupils from looking into the works of Mayow, Swammerdam, Haller, Monro, Sabatier, or any other anatomist or physiologist whatever. All *THEIR opinions* and *discoveries* are henceforth to be considered as *OUR opinions* and *discoveries*. Our will and pleasure therefore is, that their names be eradicated from the catalogue of philosophers, and that our name be substituted in their place. We alone are the only physician, and surgeon, and author: We are the staff of Moses converted into

a serpent, which has swallowed up the serpents of the magicians : We are our own supra-scapular artery, formerly unknown, but now grown so large as to annihilate all its fellows *.

Given at our Anatomical Theatre, Surgeons Square, this — day of —, — years.

We shall finish our remarks on this chapter with the following little story, which has been carefully translated from the original Greek.

“ A daw that would fain appear finer than her
 “ companions, decked herself with peacocks feathers,
 “ and all the other gay feathers that she could find :
 “ so she would not stay any longer with birds of her
 “ kind, but must needs go among the peacocks and
 “ other fine birds ; but as soon as they discovered the
 “ cheat, they fell a pulling of her : and when every
 “ bird had taken his own feathers away, the silly daw
 “ was stript to the skin, and nothing left to cover her
 “ nakedness.”

* See pages 77 and 356.

REMARKS

REMARKS

ON

Mr JOHN BELL'S ANATOMY

OF THE

HEART AND ARTERIES.

ON CHAP. IV.

THE fourth chapter, which treats OF THE PECULIARITIES IN THE CIRCULATION OF THE FOETUS, is by no means deficient in very happy and not inelegant johnbellations.

The circulation in the foetus was more than a century ago very accurately explained by Harvey; and we do not think that any addition of consequence has been made, or rather remained to be made, by subsequent authors. We shall not therefore enter minutely into our author's account of it, but content ourselves with a few remarks.

He begins with giving us reasons why the whole blood of the foetus is not sent through the lungs. "Perhaps," says he, "it might rather be contaminated there." (P. 170.) Why *there*, pray, rather than in any other part of the body?

"The ductus venosus," he says, "is the part most difficult to be understood, and never without the help of a plan." (P. 172.) Passing by the *language*, which is *barely intelligible*, we sincerely wish that

Mr

Mr Bell had given us the plan by which he himself first understood it; for neither the *plan* nor the *description* which he has given us convey any very precise ideas. Not to mention his having in his plan converted the right side of the liver into the left, and the left into the right side, we suspect that he has been somewhat misled by copying from a dried preparation; for certainly the angle at which he makes the umbilical vein and the vena portæ meet, is very different from the real state of things; and it was probably this preparation which led him (p. 175.) to suppose, that after birth the blood goes through the same vessels in a retrograde course. In his description, we are told in one place, that "the umbilical vein enters the liver at the top of the great *transverse cleft*, which divides the liver into two lobes;" and in another, that "it enters the liver at the great *longitudinal cleft*, which divides the liver into two parts." How is it possible to know the *direction* of the *cleft* from this description? In one place, we are told that the ductus venosus joins the largest of the *hepatic veins*; and in another, that it "carries the blood directly to the back of the liver, or that part which touches the diaphragm, and *there the ductus venosus enters the heart*." What are we to make of this?

Our author has displayed unusual address in his account of the circulation in the foetus. He declines all disputes, he tells us, about the nature of this circulation. At the same time he takes care to give such an account of these very disputes, as naturally leads his readers to suppose that the generality of physiologists have hitherto been mistaken. The fact is, that the

hypothesis of Mr Mery is nearly a century old ; that it was opposed at the time by Duverney and Verheyen ; that it was entirely refuted 40 years ago by Haller ; and that the account which Mr B. gives as the true one, and which he wishes to pass for new, has been, ever since the days of Harvey, the general opinion of anatomists and physiologists. Harvey has himself described this circulation with great accuracy ; and has particularly mentioned, that the two ventricles in the foetus act as one, and that both their forces are conjoined in propelling the blood through the body of the foetus.

Our author, after thus settling the circulation in the foetus, passes to the respiration in adults. " The mistake which all physiologists have fallen into," he says, " is this, *they have not observed that no creature can live with a single heart which has the oxydation of its blood performed by lungs.*" (P. 186.) If this be a *mistake*, it is the mistake of nature, and not of physiologists ; for the frog, the lizard, and many other amphibious animals, have only a *single heart*, and yet the oxydation of their blood (as our author chuses to call it) is performed in the *lungs*.

After this promising beginning, our author proceeds to *prove* that the placenta serves the foetus for lungs. This proof exhibits a very pretty individual johnbelation. The hypothesis belongs to Mayow ; but our author seems to have got it at second hand.

" One great mistake," continues our author, " runs through the whole of physiology. It has been universally believed, that the free and easy transmission of the blood was the chief use of the lungs, as if they

“ they had acted like fanners to flap on the blood
 “ from the right to the left side of the heart. They
 “ affirmed, that either continued distention, or conti-
 “ nued collapse, hindered the progress of the blood ;
 “ and they also believed universally, that if but the
 “ ductus arteriosus or foramen ovale, or any thing, in
 “ short, were left open to let through the blood, that
 “ person might live in spite of hanging, drowning, or
 “ suffocation of any kind.” (P. 188.) This is a very
 bold *unipersal johnbellation*. The fact is, that since the
 discoveries of Priestley no physiologist has been igno-
 rant that the lungs serve other purposes than those just
 mentioned by our author. Nay, the real use of respi-
 ration was suspected by some physiologists before the
 discoveries of Priestley, as any person may convince
 himself by reading the tract of Mayow on that sub-
 ject, and by consulting several parts of the writings of
 Dr Whytt. Thus our author has ascribed an opi-
 nion to all physiologists which scarcely a single phy-
 siologist has believed for at least these twenty years.

Philosophers have indeed believed, and continue to
 believe, that both the *collapse* and *over-distention* of
 the lungs oppose the easy passage of the blood through
 them ; because a great number of experiments have
 demonstrated that this is actually the case. Our au-
 thor has thought proper to deny this fact, and to af-
 firm in page 195, that the experiments so often re-
 peated by Hooke, Croone, and others, in confirmation
 of it, are not to the purpose. This assertion is suffi-
 cient to shew us what degree of attention our author
 has paid to subjects on which he decides with so much
 confidence, and to let us see how much credit his af-
 firmations deserve.

About the end of last century, the effect which compressing and dilating the lungs has upon the circulation of the blood through them, was keenly debated in the Royal Society: And the *very opinion* which Mr B. has *advanced as new*, and which he has ridiculed Dr Hooke for having opposed, was *supported by Dr Hooke* with great keenness: and the *very experiment* which our author has derided was made by Dr Hooke, in order to demonstrate that the compression or dilatation of the lungs has *no effect* whatever upon the motion of the blood through them, and that this motion is impeded by the absence of pure air, and by that alone.

Had our author consulted the 5th volume of the *Edinburgh Medical Essays*, page 806, which, as a member of the Royal College of Surgeons, he ought naturally to have read, he would have seen Dr Hooke's experiment distinctly stated, and ably refuted. And had he perused Dr Stevenson's sensible and ingenious paper, he might have escaped a great number of very awkward literary blunders into which he has fallen.

The experiment of Hooke was shown by his opponents to be inaccurate, and the experiments of Haller have since rendered it an incontrovertible fact, that both the collapse and over-distention of the lungs impede the circulation of the blood through them.

Whether the contraction and distention of the lungs by ordinary respiration be sufficient to produce such effects, is a different question. Our author decides it in the negative. "The lungs," he says, "do not collapse by expiration in any *sensible* degree." (P. 192.) Yet he allows himself in p. 170, that the collapse of

of the lungs may prevent the circulation in the lungs of the foetus; and from his own data it follows, that at every expiration the bulk of the lungs is diminished one-fifth, and that in forced expirations their bulk is diminished one-half. Now, whether the fifth part, and the half of 220, be sensible quantities or not, we leave our readers to judge. But we have another question, which Mr Bell will be good enough to answer. When a person desists from respiration, how is it that his face becomes livid and turgid? Certainly the venous blood accumulates in it, and consequently the respiration is impeded. Now what occasions this? He will say, perhaps, the blood cannot be oxydated, and therefore it cannot stimulate the heart. This does not remove the difficulty: For, in the first place, the heart still continues to act, and therefore must be stimulated; and, in the second place, there is a considerable quantity of air in the lungs, which, as we learn from the experiments of Fontana, contains enough of oxygen gas to produce the usual changes on the blood for a longer time than a person *can* continue without respiring. Why then does not the usual quantity of blood pass through the lungs? We have no doubt that our author is both able and willing to give a satisfactory answer to this question.

But our author has used another argument on this subject, which it is but fair to produce. "Is it not plain," says he, "to the meanest apprehension, that "if the blood moves *twice* through the lungs in expiration, and *twice* during inspiration; or, in other "words, if there be *four strokes* of the artery for "each respiration, and if each of the four pulses be "equally

“equally strong, that the blood passes through the lungs in all states and conditions with equal ease?” (P. 194.) Certainly, provided the *whole* blood passes twice through the lungs during every expiration and every inspiration; but if this be not the case (and it actually *is not* the case), the four pulses furnish no proof whatever.

Our author, near the end of this chapter, asserts, that Buffon, when he affirmed that puppies littered in warm milk, lived for about an hour without breathing, *imposed* upon his readers. If he had perused Haller with more attention, from whom he *has taken all the facts* mentioned in this chapter; or if he had understood better *his own new theory* of the placenta—he would have perhaps judged more favourably of Buffon. We would advise him, before he impeaches any person’s integrity again, to *repeat* the experiment which he wishes to dispute.

He had just before fallen a-laughing at Dr Beddoes for saying, that “by frequent immersion in water the association between the heart and lungs might perhaps be dissolved, and an animal inured to live commodiously under water.” The fact, however, is, that this *can actually be done* with regard to one animal, the frog; so that it is not quite so absurd as Mr Bell imagines.

As the fifth chapter contains nothing but quotations from various authors, it is not worth while to examine it; and we do not mean to enter minutely, at present at least, on the second part. Our
readers,

readers, however, are not from this to conclude, that the second part is not as rich in elegant johnbellations and profound discoveries as the first part. Almost the whole of it, indeed, is taken from Haller and Sabatier. We mention this circumstance, because we think that it is both for our author's honour and interest that it should be known; as it will prevent his reader from supposing that in many instances he is quarrelling with his own description, when he is really doing no more than quarrelling with the borrowed descriptions of others. For instance, as the full and particular description which he has given of the *external circumflex artery* of the thigh is entirely borrowed, we need not be surprised to see him adding in his own person: "But to give a more simple notion of this circumflex artery, it should be described thus," &c. (P. 468.)

After observing that the perforating arteries are extremely irregular in *place, size, and number*, our author is certainly not accountable for the particular description of their place, size, and number, which follows; as it must be evident, after what he has said, that such a description cannot possibly be his; and therefore no reader ought to be surprised when he adds: "This minute description of any *important* set of arteries never conveys any clear ideas to the reader's mind," (p. 473.), and that "there is *no artery* from the profunda downwards worth naming, not even those which I have just described." (P. 477.)

If the accounts which our author has given of the suprascapular and subscapular arteries cannot be justified on the same grounds, it must be allowed at least, that

that the bold individual johnbellations which he exhibits are, notwithstanding some flaws in them, entitled to all our admiration. We shall present our readers with his account of the subscapular artery.

“The subscapular artery is of a *wonderful* size: It is *hardly* described in books, I would say is *hardly* known to anatomists. Douglas, and *most especially* Sabbatier, have *scarcely* named it, though it is in fact *one* of the *largest* arteries in the body, being as large *absolutely* as the axillary artery from which it takes its rise.” (P. 363.) What a pity that our author was not more guarded in his expressions. A little more caution would have made this johnbellation perfect.

But Douglas and Sabatier, it appears, from his own account, have both *named* this artery, and he *might* have added too, some of its branches; and the unfortunate note at the bottom of the page informs us, that it was known to other anatomists. “It is named often,” says that note, “the scapularis inferior, or infra scapularis.” Now anatomists could not surely name, and name often too, what they did not know. They have not indeed described it as of a *wonderful* size, nor as *in fact one of the largest arteries of the body*, nor as being as large *absolutely* as the axillary artery from which it takes its rise; because it is neither *wonderful*, nor *in fact one of the largest arteries of the body*, nor as large *absolutely* as the axillary artery. Every tyro knows, that the axillary artery must always contain as much blood as the subscapularis and brachial artery together. But passing by these flaws, which we notice with regret, our author has

shewn

shewn great dexterity in never mentioning the name of the author from whom he has taken the whole of his description, we mean Haller, who has given two most excellent plates of the subscapular artery, shewing its great relative size, and its several branches, with their inosculations both on the dorsum and concave side of the scapula, and who has mentioned a great number of different authors who had described this artery before his time.

The following universal johnbellation is *wonderfully* bold : " Though the profunda is plainly the artery " of the thigh ; yet, from the ignorance of anatomists " and surgeons (*who never knew till about 20 years " ago* that there was *more than one* great artery), the " superficial artery has been named the artery of the " thigh." (P. 474.) To be sensible of the merit of this johnbellation, the reader has only to consult Haller ; who informs us, that Eustachius, and many other anatomists, had actually delineated it more than a century ago.

We allow that our author has been rather unfortunate in his attempts to make discoveries in *angiology* ; but we think that even his greatest enemies must acknowledge that he has made very considerable ones in *myology*. Were Albinus alive, how would the old gentleman blush to find, that not only a number of *new muscles*, but *new origins* and *new insertions*, of which he never dreamed, had been discovered by our author. For instance, he tells us, that " the axillary " artery is covered by the pectoral MUSCLES, because the " pectoral muscles *arise* from the CLAVICLE." (P. 359.) All that Albinus knew was, that a *part* of the pecto-

ralis major arose from the clavicle. “ The long thoracic artery is more important, supplying *all* the great pectoral muscles.” (P. 360.) Albinus has mentioned only *one* great pectoral muscle, having a fellow on the opposite side supplied by a long thoracic artery of its own.

We have given these few specimens, merely to shew the reader what he may expect from a careful perusal of the second part. Important myological discoveries, and beautiful johnbellations, grow everywhere luxuriant in the greatest abundance; and we would advise every intelligent and aspiring young man of *modest assurance*, who wishes to be at once astonished and improved, and who has any taste for the *slender*, the *delicate*, the *very pleasing*; the *great*, the *marvellous*, the *wonderful*, the *prodigious*, the *vast*, the *immense*, or the *absolutely large*—to go thither, contemplate, admire them, and gather them, to adorn his brows with the most fragrant roses of science, and to satiate his appetite with its most delicious sweets.

Nay, this second part contains a *prodigious* number of instances of the *true sublime*. The following remarks on the femoral artery, which we shall give by way of specimen, are *most beautifully* sublime and *immensely pleasing*.

“ To enumerate all the variety of accidents which
 “ may affect this artery were *impossible*; but surely,
 “ from the little that *I dare venture to say* in this
 “ place, it must seem *one of the largest*, the *most ex-*
 “ *posed*, the *most dangerous*, and by all this the *most*
 “ *important* artery IN THE BODY.” (P. 461.)

Our reader, by consulting the second part, will see
 also

also the vast importance of using the superlative degree as often as possible in all anatomical descriptions. That cold creeping sort of language, which conveys only clear and accurate ideas, can never possibly affect the fancy, and it always leaves a feeble and vulgar impression on the mind. Minute accuracy and nice discrimination ought to be laid aside: Darkness, says Burke, heightens the sublime.

With regard to our author's preface, we think it not inferior in beauties to any other part of the book. The 1st page shows how a man may write very fluently without any meaning whatever; and the 2d, 3d, 4th, 5th, 6th, and 7th, how he may talk very learnedly about philosophers, and show that all their writings contain nothing but absurdities, without giving himself the trouble to peruse these writings, or even to know the subjects of which these philosophers have treated.

As to the attack upon the anatomical nomenclature, with which the preface concludes, we agree with our author perfectly, that anatomists have sometimes from ignorance or pedantry talked in a manner not very intelligible. We are only surprised to find that he has servilely followed this nomenclature; although he has shewn in some instances, as when he speaks of the *coronary* process of the *lower maxillary bone*, that he has not always understood it.

We shall conclude with congratulating the Royal College of Surgeons in Edinburgh, upon the lustre which this noble display of our author's learning and wisdom will reflect upon it. How highly will its name be respected abroad! How superior will it appear

pear when compared with the great schools in other countries ! Foreigners will doubtless conclude, that the other members of that body are not much inferior in anatomical, and more especially in chemical, knowledge to our illustrious author. He alone has known properly how to preserve and to keep up its dignity : He alone has known properly how to appropriate the discoveries of others. And should any grumbling individual dare to complain, he knows how to reduce him to silence, by telling him, that the constitution of the Royal College of Surgeons gives to every one high privileges in speaking.



FINIS.

